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FARADAY AND SCHÖNBEIN.

The Letters of Faraday and Schönbein, 1836-1862; *with Notes, Comments and References to Contemporary Letters*. Edited by Georg W. A. Kahlbaum and Francis V. Darbishire. Pp. xvi + 376. With two frontispiece portraits. (Bâle: Benno Schwabe. London: Williams and Norgate, 1899.)

THE correspondence which has passed between the great pioneers of modern science must always be of interest to the present generation of workers. The interest is enhanced for us in the present case through the circumstance that one of the correspondents was our eminent countryman, Michael Faraday. The custom which has grown up of late years of allowing our illustrious dead to speak for themselves through their own letters is in every way a good one—particularly when the correspondence enables us to trace the history and follow the development of discoveries which have now become incorporated in the general stock of scientific knowledge. There is a living reality about a man's description of his own work, which inspires the reader to a degree quite incapable of being produced by any bald text-book statement or formal lecture-room utterance. Such correspondence is even as valuable in some cases as the original memoirs in which the final results are set forth, because we are enabled to follow the actual working out of the various lines of thought, and to stand at the elbow of the investigator as he gropes his way towards the truths which he finally gives to the world.

The present volume contains 155 letters, of which eighty-one are by Schönbein; the whole correspondence covers a period of over a quarter of a century. Some of Faraday's letters have been published in Bence Jones's life of that philosopher, but the majority have never been made public before. Much of Schönbein's correspondence also has been published, because Faraday was in the habit of communicating to the *Philosophical Magazine* results of scientific importance announced by Schönbein in his letters. But the value of the present work is in no way impaired by this circumstance, because we have now the whole correspondence brought together chronologically, the letters being left intact as originally written, and made more valuable by a most complete series of explanatory notes giving references to the papers alluded to, as well as short biographical notices of people mentioned in the correspondence. The care and trouble which has been bestowed upon this most essential adjunct to a set of letters containing references to memoirs which for us represent the scientific literature of a former generation, has thus resulted in a distinct contribution to the history of science, and the editors have laid all workers in the domains of physics and chemistry under a debt of obligation.

The human side of Faraday's nature has been so thoroughly dealt with by his biographers, Bence Jones, Tyndall and Gladstone, and, more recently, by S. P. Thompson, that very little new light is thrown upon his character by these letters. It is painful to be reminded so frequently of his failure of memory, and of his bad health leading to periodical removals from London and temporary cessation from all work. As in the case of another of our illustrious countrymen, Charles Darwin, one can only marvel at the magnitude of the labours achieved under such disadvantageous conditions.

On comparing the letters of the two correspondents, it will be found that from the social point of view both Faraday and Schönbein are equally communicative; but while the Swabian chemist gives his scientific thoughts and results in such fulness that they are in many cases capable of being published as written, Faraday, on the other hand, does not give much detailed information about his work, but only alludes incidentally to his discoveries when these appear to him to be of sufficient interest to mention to his correspondent.

We are thus enabled to follow Schönbein's work in a very systematic manner, and the development of the leading discoveries with which his name will always be associated can be traced from year to year as he unfolds them to the English philosopher. Speaking generally, it may be said that the three main lines of work which engaged his attention were the "passive" state of iron, ozone and hydrogen peroxide, and gun-cotton. Incidentally, many interesting side issues are raised, and passages can be gleaned here and there from his letters which show the wide grasp of, and philosophical insight into, the principles raised by his experimental skill. The first communication refers to "passive" iron (1836), and this subject is brought forward again and again for over six years. The explanation of the phenomenon was obscure to its discoverer, and led to his bringing the subject under the notice of Faraday and other contemporary men of science. Even if Schönbein did not find the true explanation, there can be no doubt that his work in this field had a great influence in directing his thoughts towards the action of the voltaic current and electrolysis in general, as he frequently refers to his speculations on these subjects. With regard to the explanation it may be said, as Kahlbaum points out in the introduction:—

"Even at the present day we have not succeeded in gaining clear insight into the cause from which this phenomenon proceeds."

The first reference to ozone is contained in a long letter dated April 4, 1840, in which Schönbein tells Faraday:—

"The phosphorus smell which is developed when electricity (to speak the profane language) is passing from the points of a conductor into air, or when lightning happens to fall upon some terrestrial object, or when water is electrolysed, has been engaging my attention the last couple of years, and induced me to make many attempts at clearing up that mysterious phenomenon. Though baffled for a long time, at last, I think, I have succeeded so far as to have got the clue which will lead to the discovery of the true cause of the smell in question."

This letter was communicated to the Royal Society on May 7, and an abstract published in the *Philosophical Magazine*. From that time ozone is frequently referred to, and the vicissitudes through which the new "odoriferous principle" passed can be followed with interest throughout the correspondence. At one period Schön-

bein thought that it was a derivative of nitrogen, viz. that the latter was "a compound consisting of ozone and hydrogen" (letter of April 19, 1844). In 1853 he tells Faraday that the nature of ozone appeared to have been settled in Bunsen's laboratory: "that there is one sort of ozone containing nothing but oxygen, and another that contains some hydrogen" (p. 213). By 1854 he had fully recognised that oxygen could exist in two different states, and this leads him to some further speculations on electrolysis, thermolysis and photolysis, which are well worthy of consideration even at the present time.

Students of the history of chemistry are, of course, familiar with all the points raised in the course of Schönbein's labours on ozone; but the personal recital of the discoverer's views, hopes, experimental results, and his refutation of the criticisms of other chemists will be found most instructive reading. He unbosoms himself freely to Faraday, in whom he found that sympathetic spirit which is so powerful an aid to scientific progress when exercised between co-workers whose greatness of mind and disposition exalts them above the level of all professional rivalry or petty jealousy.

The discovery of gun-cotton is heralded in 1846, when nitrated cellulose is introduced to Faraday with the statement:—

"To give you an idea of what may be made out of vegetable fibre, I send you a specimen of a transparent substance which I have prepared out of common paper. This matter is capable of being shaped out into all sorts of things and forms, and I have made from it a number of beautiful vessels." . . .

"There is another point about which I take the liberty to ask your kind advice. I am enabled to prepare in any quantity a matter which, next gunpowder, must be regarded as the most combustible substance known. So inflammable is that matter, that on being brought in contact with the slightest spark it will instantly be set on fire, leaving hardly any trace of ashes; and if the combustion be caused within closed vessels, a violent explosion takes place. . . .

"A substance of that description seems to be applicable to many purposes of daily life, and I should think that it might advantageously be used as a powerful means of defence and attack. Indeed, the congrevian rockets can hardly be more combustible than my prepared cotton is. What shall I do with that matter? Shall I offer it to your Government? I have enclosed a little bit of that really frightful body, and you may easily convince yourself of the correctness of my statements regarding its properties."

Human nature in 1846 appears to have been pretty much the same as it is now when a "utilitarian" scientific discovery is made; and in another letter of the same year he confides to Faraday, that while his knowledge of the world has been vastly increased by his experience, his "esteem for mankind has not grown in the same ratio." He adds:—

"I could tell you a great many things of an incredible description, but I will not trouble you with detailing facts which I should like never to have become acquainted with myself. So much, however, I must say, that by the occurrences alluded to, my temper, which is usually not much liable to be ruffled, and the placidity of my mind have been suffering these many months" (p. 165).

But apart from these capital discoveries with which Schönbein's name will be always linked, and which are now part and parcel of our modern science, some of the bye-products of that active mind are perfect marvels of scientific intuitiveness. A few of these collateral suggestions have been noted in reading through the correspondence, and readers of NATURE will be interested in having their attention called to some of the more striking passages. Compare, for instance, the present views on the nature of electricity with the statement written to Faraday in 1839:—

"It appears to me that what we call static electricity is only a state of tendency of something to move in certain direction, and that current-electricity is the actual motion of that something. That motion must not be considered as one of weighty particles, but as a motion of something that is not affected by gravity; as a peculiar motion of the ether, if you like. According to these hypothetical views, we can easily conceive how a vibratory motion might be propagated through a space, or medium, empty of weighty particles, but filled up with some imponderable matter which is capable of being brought into a moving state. The only thing difficult to conceive is the relation of that imponderable agency to the weighty particles in their natural and excited condition; that is to say, the way in which both are acting upon each other. It is possible that a state of tendency to motion may be brought about in ether only by a peculiar action of ponderable particles upon that fluid, and that consequently such a state cannot exist in it without the presence or agency of matter, whilst moving ether of itself has the power to impart motion to ether being at rest" (p. 71).

The question of the colour of oxy-compounds appears to have directed Schönbein's attention towards the subject of colour in general. In 1852 he penned this most significant statement:—

"I cannot help thinking that the colours of substances, which up to this present moment have been very slightly treated (in a chemical point of view), will one day become highly important to chemical science, and be rendered the means to discover the most delicate and interesting changes taking place in the chemical condition of bodies. In more than one respect the colour of bodies may be considered the most obvious *signatura rerum*, as the revealer of the most wonderful actions going on in the innermost recesses of substances, as the indicator of the most elementary functions of what we call ponderable matter."

The letter from which the above passage is quoted contains remarks which—to put the case with the least disparagement to the memory of their writer—show that the then newly developing science of organic chemistry found very little to commend it to Schönbein's mind. Faraday echoes his sentiments in his reply to this letter, in which he says:—

"You are very amusing with your criticisms on organic chemistry. I hope that in due time the chemists will justify their proceedings by some large generalisations deduced from the infinity of results which they have collected. For me, I am left hopelessly behind; and I will acknowledge to you that, through my bad memory, organic chemistry is to me a sealed book."

Again and again does Schönbein declare his attitude towards this branch of science, not only in his letters to Faraday, but also to other contemporary men of science. In a letter to Faraday, written in 1854, he speaks of "cook-like chymists, who are brewing on and on their liquors and puddings without paying much attention to the conditions of the primary matters they are continually

mixing together." Even the editor of the present volume finds it desirable to append a sort of apologetic note (p. 225) concerning this "harsh verdict," and pointing out that, although concerned with the chemistry of only one element, this branch of the science has had "great practical value and importance." It is not to be wondered at that Schönbein should have felt some trepidation in meeting his great compeer Liebig, the father of modern organic chemistry, which event is graphically described in a letter written to Faraday in 1853 (p. 216) :—

"Of course, I met Liebig at Munich, whom I knew before little more than by sight, but within the first five minutes we had found out the footing upon which both of us could move comfortably enough. You will laugh when I tell you Liebig asked me to deliver a lecture before a very large audience in his stead, and Mr. Schönbein, though reluctantly, yielded to that strange demand. The subject treated was that queer thing called 'Ozone,' which ten or twelve years ago, as you are perhaps aware, was declared by a countryman of yours and pupil of Liebig's to be a 'nonens.'"

Could the shade of Schönbein revisit the laboratory of a modern worker in organic chemistry, he would find that the latest "Handbook" consisted of four large volumes, containing altogether some 6000 pages of closely printed matter, all compiled by one man (Beilstein). But nature, which endowed the "mighty atom" of carbon with such marvellous potentialities, had her revenge upon the illustrious Swabian during his lifetime, for she placed in his way a discovery which, curiously enough, is just now exciting the greatest interest, viz. the oxygen-carrying power of certain enzymes known at the present time as "oxydases." His first allusion to this is in 1855, when he wrote to Faraday :—

"You know that I entertain a sort of innate dislike to touch anything being in the slightest way connected with organic chemistry, knowing too well the difficulty of the subject, and the weakness of my powers to grapple with it ; but, in spite of this well-grounded disinclination, I have of late, and as it were by mere chance, been carried in the midst of that field, upon the intricacies and depths of which I have been used all my life to look with feelings of unbounded respect and even awe. The picking up of a mushroom has led to that strange aberration of mine, and you will ask how such a trifling occurrence could do that. The matter stands thus : What the botanists tell me to be called *Boletus luridus*, with some other sorts of mushroom, has the remarkable property of turning rapidly blue, when their hat and stem happen to be broken and exposed to the action of atm[ospheric] air. On one of my ramblings I found a specimen of the said *Boletus*, perceived the change of colour alluded to, and being struck with the curious phenomenon, took the bold resolution to ascertain, if possible, its proximate cause."

He then describes his experiments in some detail, and comes to the conclusion that this and other Fungi contain an "organic matter" which is "a true carrier of active oxygen." This letter was communicated to the *Philosophical Magazine*, and published in vol. ii. 1855.

As another example of Schönbein's power of grasping and dealing with scientific problems, we may refer to his treatment of "polarisation," which term he used in at least two senses, viz. the electrical sense in which it is now used as indicating the reversal of current by charged electrodes (1838), and later (1859) to indicate "two active kinds of oxygen standing to each other in the relation of + to −." This association of ideas in the philosopher's mind is a good instance of pre-vision, and his remarkable comparison of the opposite states of the two kinds of oxygen to Pasteur's racemic combination of the two tartaric acids (p. 288) is a bold analogy which may even yet find justification. This explanation of voltaic polarisation, given as far back as 1839, is substantially the same as that adopted at the present day. So also his views on the course of chemical change, expressed in a letter to Faraday in 1856, are so much in harmony with modern notions that they are worth emphasising by quotation :—

"Another fact, not quite void of scientific interest, is this, that in some instances I can show, as it were, steps which the oxidation of certain matter passes ; . . . it is not impossible that any oxidation is a sort of chemical drama, consisting of different acts, the last of which is real oxidation. . . . Schönbein maintaining that between the moment on which two isolated elementary bodies meet, and that of their chemical associating being finished, there lies a whole world of phenomena, and is very much of which the chemists of the present day have as yet not the slightest notion. There is even within inorganic chemistry something which I might call physiology, and the most interesting and truly scientific object of chemical research lies, to [in?] my opinion, within the short interval of time alluded to, and hence the great difficulty of such an investigation" (p. 271).

The next paragraph in this letter mentions, by the way, a synthesis of formic acid by the oxidation of olefiant gas by ozonised oxygen.

Among the other numerous subjects discussed in the course of the correspondence, "contact action" may be mentioned. In sending a pamphlet to Faraday, published in 1844 by Schönbein, the latter says :—

"There is also a paper in the book treating of chemical effects produced by contact, on which I should like very much to have your opinion. Having these many years entertained strong doubts about the correctness of the atomic theory, and been inclined to consider what is called a 'molecule' of a body *as a centre of physical forces* (italics ours), I have tried to make that view bear upon the chemical actions being produced by contact."

So that we have here the Boscovich notion very clearly set forth. The same letter also contains a paragraph which will go to the heart of many and many a worker in science who reads this notice :—

"Having had no less than nineteen hours to lecture a week in the course of this winter, you may easily imagine that I had no time for making researches : I grow, indeed, impatient of that everlasting schoolmastering, and am longing for being placed under circumstances more favourable to scientific pursuits."

In selecting specimens of the correspondence from the volume before us, we have necessarily given Schönbein the greater prominence. Faraday, as already explained, was not so communicative of his scientific results. The latter, moreover, may be assumed to be more familiar in this country than the original papers and memoirs of the Swabian chemist. But scanty as are the English philosopher's references to his work, the chronological sequence of his main discoveries can be traced, and these

appear to have been eagerly followed by his correspondent. Of particular interest at the present time is Faraday's statement, in 1845, that he had failed to liquefy oxygen at -140° F. under a pressure of 60 atmospheres; and in 1852 he asks Schönbein:—"Have you condensed oxygen?—I wish you could tell me what liquid or solid oxygen is like. I have often tried to coerce it, and long to know."

In November 1845 he mentions one of his fundamental discoveries to Schönbein in these words:—

"I happen to have discovered a direct relation between magnetism and light, and also electricity and light, and the field it opens is so large and I think rich, that I naturally wish to look at it first" (p. 148).

Another little list of scientific gossip concerning Crosse's supposed production of insects by an electric current (p. 33) will be found of interest, as also the reference to table-turning (p. 214), concerning which he says:—

"I have not been at work except in turning the tables upon table turners—nor should I have done that, but that so many inquiries poured in upon me that I thought it better to stop the inpouring flood by letting all know at once what my views and thoughts were. What a weak, credulous, incredulous, unbelieving, superstitious, bold, frightened, what a ridiculous world ours is, as far as concerns the mind of man. How full of inconsistencies, contradictions and absurdities it is" (p. 215).

The above and other passages in this letter come as near to misanthropy as anything to be found in Faraday's correspondence. It is obvious from the context that the letter (July 1853) was written during one of his periods of prostration, for he says: "Consider my age and weariness, and the rapid manner in which I am becoming more and more inert."

The extraordinary pertinacity displayed by Schönbein in following up his ideas concerning the "polarisation" of oxygen, and in searching for the hypothetical "antozone," is well brought out in the course of the correspondence. In 1860 he thought he had obtained "antozone" from fluor-spar, and he described his experiments to Faraday, who in his reply raises a question concerning nitrogen in a very remarkable passage:—

"But surely these wonderful conditions of existence cannot be confined to oxygen alone. I am waiting to hear that you have discovered like parallel states with iodine or bromine, or hydrogen and nitrogen. What of nitrogen? Is not its apparent quiet simplicity of action a sham?—not a show, indeed, but still not the only state in which it can exist. If the compounds which a body can form show something of the state and powers it may have when isolated (as in your ⊖○⊕), then what should nitrogen be in its separate state?"

The extracts which have been given are sufficient to show that the editors of this volume have made a most valuable contribution to the literature of science. It is out of such materials as Dr. Kahlbaum and his colleague have now provided that the history of the science of the nineteenth century must be built up; and we are glad to have received from this same author other volumes giving the correspondence of Liebig and Schönbein, and a monograph on Schönbein's work, which we hope to notice in due course. Certainly this period of six-and-twenty years during which the intimacy between Faraday and Schönbein began and ripened into the warm friendship which was terminated only by the death of one of the correspondents was one of extraordinary activity and productiveness. The names of the contemporary workers referred to comprise, not only those already mentioned, but also Arago, Berzelius, Becquerel, St. Clair Deville, Frémy, Houzeau, Marignac, De la Rive, Hofmann, Magnus, Nobili, Pasteur, Pélouze, Pettenkofer, Plücker, Poggendorff, Rose, Wiedemann and Wöhler on the Continent; Draper in America; and in this country Airy, Andrews, Brodie, Daniell, Grove, Herschel, Noad, Stokes and Whewell. It is interesting to read that in 1856 Faraday sent a volume of his researches to Schönbein through "Mr. Roscoe, a student under Professor Bunsen at Heidelberg"; while in 1843 he refers to "a magnificent steam electric apparatus" made by Mr. (now Lord) Armstrong.

With respect to the manner in which the editors have performed their task, there are some points to which attention may be called. Dr. Kahlbaum lays down the principle in the preface that in publishing historical documents these

"should be set forth in exact agreement with the original, and in the next place provided with as many suitable comments as possible to explain their meaning."

Regarding the latter statement, we have already pointed out how admirably the editors have done their work. In accordance with the first statement, the letters have been printed, on the whole, exactly as written, and thus any "editing" which was done for the *Philosophical Magazine*, or for the Royal Society, can now be compared by the curious with the original documents. In no case, as far as we have ascertained, has Schönbein's meaning been altered by the editorial process. It must be remembered that Schönbein thought in German and wrote in English, and the letters generally show that his English was excellent. Only in matters of spelling and the use of capitals is there any laxity to be found, and it is perhaps to be regretted that the editors did not leave every word of Schönbein's intact or else have adopted a uniform system of correction throughout. Thus, where they alter the spelling it is generally by interpolating some trivial correction, such as "favorite" to "favo[u]rite," "Alps" to "Alp[e]s," "color" to "colo[u]r," and so forth, while "oxigen," "sulfate," "british," "french," "german," "You," &c., are allowed to pass. The whole result leaves an impression of straining at gnats and swallowing camels; particularly when such spelling as "oxigen" and "german" appears also occasionally in Faraday's letters, possibly through misprints. The volume, we may add, has been printed at Bâle. Faraday's writing also was in some cases so illegible that the editors have been obliged to leave gaps or to suggest interpolations. The portrait of Faraday is the well-known one prefacing Bence Jones's "Life," and that of Schönbein is drawn from a statuette of 1855 and contemporary photographs. The original letters are now, though the generosity of Faraday's niece, Miss Barnard, and Schönbein's daughters, deposited in the University Library at Bâle.

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